criticised by his enemies for things which were deserving of highest praise and especially praised by his friends for things which were unfortunate lapses from scientific accuracy. I should consider it both unwise and unfair to him to specially rest his reputation in aerodynamics upon the so-called Langley Law, or upon the computation which gave rise to it, as they do not seem to represent his best work. The particular computations which led him to enunciate this law are found on pages 63-67, 'Experiments in Aerodynamics.' A careful reading shows that he never actually tried the experiments of which he professed to give the result. . . . It is clear from the Doctor's statement that he never demonstrated by direct experiment that weight could be carried at the rate of 200 pounds per horse-power at 20 meters per second, nor that the power consumed decreased with increase of speed up to some remote limit not attained in experiment. He merely assumed that he could have done it by varying the experiments a trifle and based the so-called Langley Law on this mistaken assumption."

The Regents of the Smithsonian Institution adopted this suggestion and the Langley Law was not inscribed on the tablet.

The article in NATURE of November 3 states that "the Wright Brothers are equally clear in their acknowledgment of Langley's work," and gives a quotation from them to support this idea. This quotation, taken in connection with the suggestion of the writer in NATURE, may have carried to some readers the erroneous impression that the Wright Brothers acknowledged an indebtness to Langley for his scientific work. This was not the fact. The quotation given makes no reference whatever to Langley's scientific work. It is simply a generous acknowledgment by the Wrights at the time of Langley's death for the inspiration received from his faith in the possibility of human flight, and containing an expression of gratitude for information as to books on the subject of flight other than those they had already read. The Wright Brothers have also acknowledged their indebtedness to Chanute, Mouillard and others, but have always made it clear that their greatest debt was to Lilienthal.

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SOME difficulty is felt in continuing a discussion of the relative merits of the great pioneers in aviation, Prof. Langley and the Wright Brothers, since they are all entitled to our esteem, and comparison seems to be unnecessary. Mr. Griffith Brewer does not appear to dissent from such a general statement, but one suspects that his enthusiastic admiration for the work of the Wright Brothers has led him to make extravagant claims.

It is very surprising to hear that "the Wright Brothers first established a scientific basis for aeroplane design," and that their laboratory measurements "covered a field many times greater than had been covered by the work of all other experimenters together." The only publication cited in support of this contention occurs in two pages of the *Century Magazine* in 1908, and readers of scientific literature in aeronautics will realise that they do not know where to look for data based on the work of the Wright Brothers. Indeed, Mr. Brewer indicates that this must be so when he says, "While in Dayton (in 1914) I was allowed to examine, with the privilege of copying, much of the personal correspondence and diaries, as well as the records of the early purely scientific work of the Wright Brothers"; apparently the work was not publicly available. Is it then strange that one should look to Langley as the scientific pioneer, since he took the normal steps of a man of science and published complete accounts of his results as he obtained them?

Mr. Brewer refers to the "Langley Law" that the faster an aeroplane be flown the less will be the power required to sustain it. He says: "The fallacy of this law is well known to all aeronautical engineers to-day, but up to 1910 this was generally accepted as Langley's chief contribution to the science of aerodynamics." The inadequacy of the law is evident now, but it is still at least partly true; in the case of the most modern aeroplanes the horsepower for flight decreases as the speed increases from the least at which support can be obtained. The increase of power required to increase the speed of the modern aeroplane above a certain limit is due to the light-weight engine, a factor which did not come into consideration in early practice. The error of unsound extrapolation outside the experience of the day was made, but only superficial observers could regard the enunciation of the law as "Langley's chief contribution" to aeronautical research.

One can only disagree with Mr. Brewer in his review of the situation and regret that this aspect of pioneer work in aviation was introduced in the tone of the paper on "The Langley Machine and the Hammondsport Trials." The point of the paper was not so much missed, as suggested by Mr. Brewer, as countered owing to the fact that the statements therein did not carry conviction. One of the articles in NATURE intimated this in the suggestion that the Royal Aeronautical Society should take up the matter and after full investigation issue an official report. The views on the Langley aeroplane expressed by Mr. Brewer cannot be accepted as final although given in all good faith.

THE WRITER OF THE ARTICLES.

Some Biological Problems.

DR. CUNNINGHAM (NATURE, February 9, p. 173) cannot be more weary of this discussion than I. It is many years since I, becoming doubtful, first tried to discover the precise meaning of certain biological key-words. To this day I have not succeeded. It has been my misfortune to encounter authoritative people who, instead of perceiving that I was genuinely puzzled, thought I might do "much harm by leading many who have no special knowledge of heredity and evolution"—*e.g.* Professors Goodrich and Bayliss—"to distrust the work of those who are engaged in research on these subjects." May I suggest that in this matter authority and regard for public opinion are out of place. Most biologists profess to know the meanings of their terms; but there is no agreement, and no definitions can be framed which cover the whole of common and accepted usage. A science which lacks a precise and significant means of expression labours under paralysing difficulties.

Dr. Ruggles Gates thinks that a variation is a character. Surely he is mistaken. When one individual varies from another (*e.g.* child from parent) the difference is revealed in a character. If this new character becomes established in the species, it remains a character; but, even colloquially, it ceases to be a variation. How then can a variation be a character? A variation cannot be thought of without a comparison, explicit or implicit, between two separate individuals; a character can always be thought of without such comparison. Evidently, then, a variation is not a character, but an unlikeness between two individuals which is displayed in a character. When we say that a variation is innate *or* acquired we know exactly what is meant—*i.e.* that a difference between two individuals is germinal *or* somatic, a product of nature *or* nurture. For example, if B differs from A in that he has a sixth digit and a scar, he varies innately (not by acquirement) in the case of the digit, and by acquirement (not innately) in the case of the scar. But when we say that the digit itself is innate (not acquired) and the scar itself acquired (not innate) what can we mean? We are now comparing not separate individuals, but two characters of the same individual. Obviously the digit as such is no more germinal, no more a product of nature and evolution, no less somatic, no less a product of nurture than the scar. How then is the one more innate or acquired than the other? Our terms "innate" and "acquired" are now unmeaning or else they have new meanings. But, as I say, no new meanings can be thought of which cover the whole of established usage. All this would not matter were it not for its consequences.

When it is said that an innate variation is inheritable we know exactly what is meant—*e.g.* that the descendants of B will tend to reproduce the digit (will tend to differ from A) even when reared under the same influences as A. Again, when it is said that an acquired variation is not inheritable, we know exactly what is meant—that B's descendants will not reproduce his scar when reared under the same influences as A. But when it is said that innate characters are inheritable and that acquired characters are (or are not) inheritable, what is meant? Either the word inherit has now no meaning, or in this single sentence it has two directly contrary meanings—inherit when applied to innate characters, and non-inherit (*i.e.* vary) when applied to acquired characters. We are now fully immersed in that fog of words in which, except for a brief interlude in Darwin's time, biology has strayed for a century.

The trouble began in the popular notion that, like Topsy, some (*i.e.* innate) characters "just growed," while others are acquired through some influence or other and may become, through "centuries" or "generations" of experience, "innate" in the race. Lamarck formally introduced this popular notion to science. His second law has been disputed and shown to be inconsistent with the first, but the first has been accepted without question. Yet it is crammed with obviously erroneous assumptions. It is not true that "In every animal that has not passed beyond the term of its development, the frequent and sustained use of any organ strengthens it, develops it, increases its size, and gives it strength proportionate to the length of time of its employment. On the other hand, the continued lack of use of the same organ sensibly weakens it; it deteriorates, and its faculties diminish progressively, until at length it disappears." (1) No character is in any clear sense of the words more innate or acquired than any other. (2) In our own bodies are many characters—*e.g.* hair, teeth, external ears and genitals—which do not develop in the least in response to use, or atrophy in the lack of it. Only some characters develop in response to use, and only such characters atrophy in the lack of it. There is no evidence that the development of any characters in low animals is influenced by use. On the contrary, the power of so developing appears to be, relatively speaking, a late and a high product of evolution. But as to that I shall have something to say when trying to trace evolution from the physiological standpoint. (3) Lamarck's first law dimly implies that which is more clearly implied in the writings of succeeding biologists—that although all characters develop somewhat in response to use, no characters develop greatly in that way; the actual truth being that, from birth onwards, much the greater part of the growth of the higher animals is made in response to that influence.

For millions of years Nature fashions a species to develop in response to an influence (*e.g.* injury or use). The race persists because its individuals grow in that way when need arises. At long last a biologist observes such a character—*e.g.* scar, or blacksmith's muscles. For no particular reason he calls it "acquired," and supposes that evolution results from the "transmission" of such traits—a wonderful thought, for he must know that regeneration and use-acquirements are products of evolution. For half a century his fellows agree. Then some one denies not the fundamental error, not the special acquiredness of the character, but only its inheritability. Thereafter, controversy, founded in the best scholastic style on three misused words and a number of unverified assumptions, rages for half a century. Presently a majority are convinced that acquired characters are not transmissible. Thereupon some biologists devote their energies to discovering what characters are innate and therefore inheritable, and others to discovering what characters are inheritable and therefore innate. Hedged about by her extraordinary terminology, biology becomes isolated from a number of kindred sciences and studies—physiology, psychology, medicine, history, pedagogy, and the like. Of what use is it to the students of these studies to learn that a character is innate or acquired? They want to know what causes it to develop. Of what uses is it to the biologist to know how a character develops? He wants to know whether it is innate or acquired. Meanwhile many problems, mainly psychological, social, and medical, of vast importance, on which the whole future of the race depends, await solution and the driving home of the truth by the weight of scientific proof and united scientific conviction. But all these problems are too big for the subsidiary sciences. Their students are too few in number. Moreover, in every case the evidence is derived from more than one science. Only biology, which sits at the hub whence radiate all the studies that deal with life, is in a position to deal with them, and then only if it has a clear and precise medium of expression. I daresay many biologists think I am vapouring. But, if they wait, I think they may perceive a method in my madness. This much I will permit myself to say: that unless biology awakens from her long sleep, our modern civilisation is likely to smash, just as old Rome smashed and for the same reason—because there is not enough intelligence left to run a society grown very complex.

According to Dr. Cunningham, biologists bestow the descriptions "innate" and "acquired" on characters which develop in response to internal and external stimuli respectively. That is to say, "innate" and "acquired" are supposed by him to be technical terms which have meanings quite other than their ordinary dictionary meanings. But:—

(1) Before this discussion began no one ever thought of such technical meanings. On the contrary, as attested by all literature, biologists have genuinely believed that some characters are really innate and others really acquired. Hence the synonyms—germinal, blastogenic, plasmogenetic, somatic, somatogenetic, and the like—which were coined to give definition and emphasis to this belief.

(2) Light, heat, moisture, gravitation, food, injury, and the like are all external influences; and of them, the only one which is commonly regarded as evoking acquired characters is injury. On the other hand, hormones and functional activity (use) are internal influences; and since hormones act from outside the characters they influence, whereas use acts from within, the latter is the most internal of all stimuli. But use is supposed to be more especially *the* influence

which evokes acquired characters. Clearly internal, just as much as external, stimuli evoke what are called "acquired characters."

(3) As a fact, biologists when classifying characters as "innate" and "acquired" have not in practice been influenced by the kind of stimuli which have evoked them. As again all literature testifies, they have, in accordance with popular usage, called all characters which develop in response to any very glaringly obvious stimulus acquired. For example, they do not call the musculature of the child, the youth, and the ordinary man "acquired"; but they do bestow that name on the musculature of the blacksmith, though the latter develops in response to precisely the same influence. Any number of similar examples might be named.

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February 25, 1922.

A Rainbow Peculiarity.

EVERY one has observed a brilliantly coloured rainbow and also the secondary bow situated some distance on the outside of the primary. Is it a fact of general observation that the whole area of the inside of the primary bow is brighter than the region outside?

It was not until the winter of 1913 that this bright inner region was brought to my notice on a photograph I secured on December 1 of that year. On the morning of that day one or two heavy showers passed from S.W. to N.E., traversing the ground to the north of the Norman Lockyer Observatory. The primary bow in question was distant about one mile to the north of the observatory, as could be judged from the spot where the rainbow ended on the ground. An attempt was made to photograph the bow with a 4×5 screen focus Kodak using the ordinary Eastman film, and on development it was found that the light from the interior area of the bow had acted on the film in a much more actinic manner than that on the outside—or, in other words, the area inside the bow was brighter than that on the outside. The accompanying illustration (Fig. 1) gives a reproduction of the photograph.

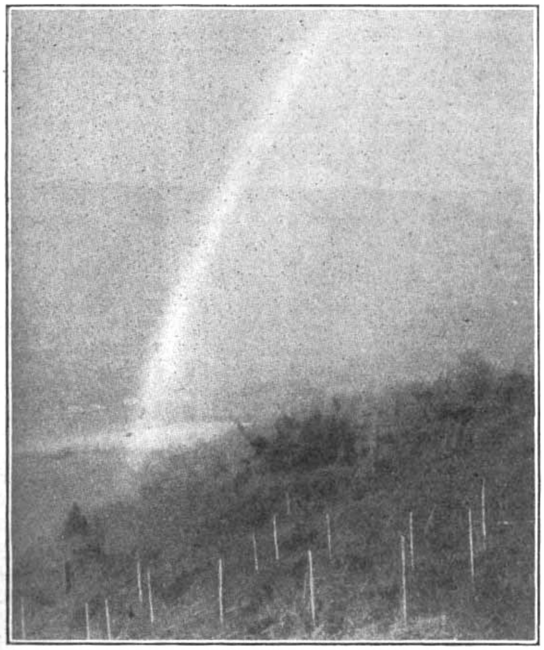

FIG. 1.—Rainbow photographed on December 1, 1913.

I somewhat doubted the reality of this appearance until I had taken another photograph of a similar nature, and an opportunity occurred in December of last year.

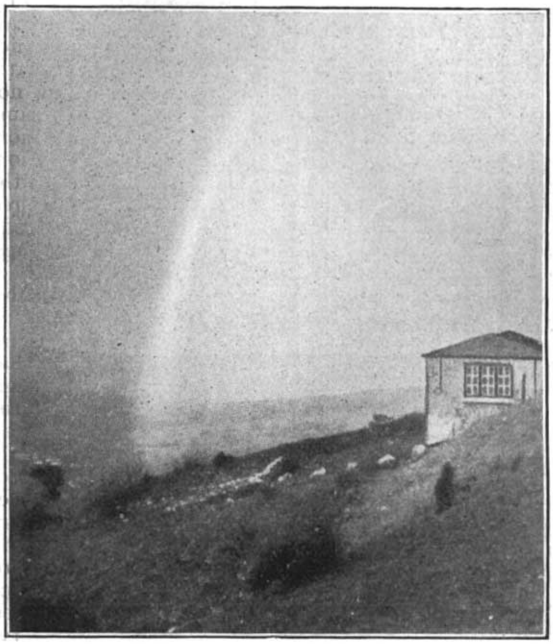

FIG. 2.—Rainbow photographed on December 22, 1921.

On December 22 numerous showers were passing to the northward of the observatory, and I photographed three different rainbows during the morning. They were not so brilliant as that photographed in 1913, but yet sufficiently bright to record the same phenomenon. One of these photographs is reproduced in Fig. 2, and a comparison of the intensity of the distant landscape inside and outside the primary bow corroborates the previous photograph.

These photographs thus establish a fact in Nature which appears to have been rarely noticed visually. Kämtz in his "Lehrbuch der Meteorologie" (vol. 3, p. 158), a book which was published so long ago as 1836, writes on the subject as follows:—"When a rainbow with very pronounced colours is projected against a dark cloud, the sky above the first bow is darker than that underneath. If we follow the path of light in our spherical drops and remember the limiting values which have been given above, we receive none of the inner surface reflected rays from any drops which lie higher than those in which we found the maximum and which form the bright bows; lower-lying drops also send out rays from the inner back surface, and, although these more or less diverge, they tend to produce an undoubted brightness under the bow. The drops lying above the bow also send out reflected rays from their near sides, while from the drops lying under the bow we receive rays from the far side."

Following this extract Kämtz states that he has to thank Brandes for directing his attention to this phenomenon, and gives a reference to Gehler's "Wörterbuch," *Nach. Astr.*, vol. 7, p. 1324.

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